

Pioneer Profiles: An Interview with Don Baer

Michael D. Wesolowski
Florida Department of Children and Families

This is an interview with Donald M. Baer. The interview includes discussion of his education at the University of Chicago, his work at the University of Washington and the University of Kansas, events that influenced his career, and his perspectives on various issues. His accomplishments include developing the standards for the practice of applied behavior analysis, creating an empirical research base for language training for people with severe disabilities, initiating procedures that led to generalized imitation, formulating experimental designs for applied behavioral research, and devising procedures for generalization and maintenance of behavior.

Key words: Donald M. Baer, pioneer, interview, behavior analysis, applied behavior analysis

Donald Merle Baer was a true pioneer in applied behavior analysis. In a seminal article with Montrose Wolf and Todd Risley, Baer both defined applied behavior analysis and set the standards for its practice (Baer, Wolf, & Risley, 1968). The article described applied behavior analysis as applied, behavioral, analytic, technological, conceptually systematic, effective, and capable of generalized outcomes; defined those terms; discussed the importance of reliable data; and described two single-subject experimental designs—the reversal design and the multiple-baseline design.

Baer taught us much about the scientific application of behavior analysis. He explored research methodology and formulated new experimental designs, such as the multiple-baseline design (Barton, Guess, Garcia, & Baer, 1970), the multiple-probe design (Horner & Baer, 1978), and the use of multielement designs to study interaction effects (Hains & Baer, 1989).

Baer's influence was pervasive in a number of areas. With his graduate school adviser, Jacob L. Gewirtz, Baer

conducted a series of studies on the effects of deprivation and satiation of social attention (Gewirtz & Baer, 1956, 1957, 1958a, 1958b; Gewirtz, Baer, & Roth, 1958). This was truly pioneering work because it preceded articulation of the concept of establishing operations.

With Peterson, Sherman, and others, Baer studied generalized imitation (Baer & Deguchi, 1985; Baer, Peterson, & Sherman, 1967; Baer & Sherman, 1964; Garcia, Baer, & Firestone, 1971). Generalized imitation made teaching people who were profoundly developmentally disabled faster and easier.

With Stokes, Baer showed the need to program generalization rather than to simply teach skills and assume that they would transfer to other settings and be maintained over time. Furthermore, he contributed to the development of blueprints for a technology of generalization (Baer, 1981; Stokes & Baer, 1977; Stokes, Baer, & Jackson, 1974; Stokes, Fowler, & Baer, 1978).

It is common to find at least one or two workshops on functional communication training at behavior analysis conferences. The value of language training for individuals with severe disabilities cannot be overstated. It is not surprising that Baer and his colleagues established a foundation for those efforts with their extensive research on language acquisition and training (Baer

Correspondence should be addressed to M. D. Wesolowski, Florida Department of Children and Families, Behavioral Services Unit, Suite N-812, 401 NW 2 Ave., Miami, Florida 33128.

Editor's note: Authors considering submission of interviews should contact the editor first. Publication of future interviews will be by invitation only.

& Guess, 1971, 1973; Guess & Baer, 1973; Guess, Baer, & Sailor, 1978; Guess, Sailor, & Baer, 1976; Guess, Sailor, Rutherford, & Baer, 1968; Mann & Baer, 1971; Sailor, Guess, Rutherford, & Baer, 1968). Baer is also known for his contributions in the area of child development. He and Bijou wrote three books on child development from an operant perspective (Bijou & Baer, 1961, 1965, 1967). These books served as primers for many students of behavioral psychology.

Baer's participation in this interview was enlisted in March 1998. The interview process took nearly 4 years; I wrote the questions and mailed them to Don, and he wrote his responses and mailed them back. Although this process took a great deal of time, it preserved the integrity of the written word. The interview began with some questions about Don's education.

You did your undergraduate and graduate work at the University of Chicago. Do you think getting all your degrees at one university is a bad practice because you learn from the same professors at all stages of your education?

I fell in love with the University of Chicago within a few weeks of entering it in 1948, which was just after my junior year in high school. That high school was very weak and small, and I hated being there for two reasons: (a) I was the only Jew in my class, a fact my schoolmates often stated as their supreme insult. (b) I was in love with learning, and I seemed to be the only one in my class of that kind as well, a fact they stated as their second-best insult. The school was just good enough to show me that there was a world of knowledge, scholarship, and understanding; but not good enough to open the really good parts of it to me.

Late in my sophomore year, I discovered that the University of Chicago admitted students prior to their high school graduation, and that the agencies that rate universities usually considered Chicago one of the best six in the nation. From that moment, I was

consumed by the wish to go there immediately.

That wish created a family crisis: My just-barely-middle-class parents, Belorussian immigrants without high school education, had always insisted their children would go to that American mystery called "college." But my father saved the necessary money on the premises that it would happen after the senior year of high school, and would be at the very cheap state university. I was bitterly disappointed, but I agreed with my father: His plan was both generous and the only realistic one. Whereupon my mother, for the first time in her married life, got herself a selling job, explaining to my outraged father that they would need the extra money to send me to the very expensive Chicago, and a year early at that. My father, who spent his life clawing the family's way into the middle class, could not countenance his wife working; he finally agreed with her that I could go to Chicago a year early, on condition that she give up this shameful idea of a middle-class wife working. (That was, of course, her plan all along.) They had little money at best; his decision meant they would go back into their survival lifestyle (learned the hard way during the Depression) as long as I was at Chicago.

I tell you all this to make clear that I saw the University of Chicago as my greatest dream, granted to me against all the odds and all practical considerations, by an ongoing act of great sacrifice by my parents. Whatever it was worth objectively, and that was a lot, to me it was worth much more, because of what it cost my family for me to be there. Small wonder I adored it.

Besides, when I arrived, I found it was much, much better than I had dreamed it would be. As an undergraduate institution, it was indeed nothing but a world devoted to knowledge, scholarship, analysis, and understanding, taught by people who were extraordinarily good at that, to people who wanted little more than to consume it and discuss it endlessly, and do

it themselves. And I was surrounded by Jews—so many that Jewishness was no longer even an issue.

Of course I would stay as long as I could. Especially because after the first 2 years, I found ways to achieve scholarships, fellowships, and part-time work sufficient to pay my costs myself.

I think that university people who say their students should not get all their degrees from that university are simply saying they don't think their university is a very good one. I thought Chicago was an excellent university. Fortunately, none of my professors told me it was a bad one.

If I had known I wanted to study behavior analysis, my rational postgraduate path would be to enter the university with the best program in behavior analysis. But I didn't know that after my baccalaureate graduation—and if I had known it, there wasn't in 1950 such a university. There were only a few universities that had at best one or two operant psychologists. It turned out that Chicago had two: Howard Hunt and Jacob Gewirtz, both self-taught. So it was indeed a very good choice in retrospect, even if it was not chosen for that reason. All I knew at that moment of deciding to stay for graduate study was that I wanted to study behavior as if it were a natural science. Chicago was full of natural scientists, full of philosophers of science who understood very well the difference between natural science and other approaches, and very relaxed about students crossing departmental lines to learn what they wanted. So in prospect as well as in retrospect, it was a very good choice to stay.

Why did you choose psychology as a subject matter to study?

I don't know why. I can only report that I wanted to understand the world by applying natural science to it. The university said there was a science of behavior called psychology, and the phrase implied the most important thing natural science could ever understand was how people behaved. (I had noticed Aristotle's claim that politics

was the most basic science, because it was the science of how societies behaved, and because how we behaved determined not only what we would learn in the other sciences and how we would apply that, but indeed would determine if we even got to study those other sciences.)

Unfortunately, the university's dominant undergraduate examples of psychology were essentially psychoanalytic—obviously not natural science. As a graduate student, I turned to mathematics instead. I was not liking much of it when a friend in graduate-level psychology asked me to explain an equation in his textbook—the Weber-Fechner law. I was intrigued that a psychologist—a psychoanalyst, surely?—would ever have to deal with an equation, especially one relating the discriminability of stimulus differences to their magnitudes. My friend denied my imputation that he was psychoanalytic, argued that how stimulus magnitude controlled stimulus discriminability was basic behavioral science, and showed me his experimental psychology text, by Postman and Egan. Stunned by its mere existence, I borrowed the book, stayed up all night to read it through, and, finishing just as the sun was rising (such symbolism!), said aloud to myself that I would study experimental psychology rather than mathematics.

Does that explain why I chose psychology? Would you prefer a multiple-baseline design, in which a series of young natural scientists almost committed to other sciences are shown, at different points in their baselines, the possibility of an experimental natural science of human behavior? The question would be how many of them suddenly would shift their efforts to something like the experimental analysis of behavior. If they did, would we have an explanation? The explanation would be only that some of us find the possibility of a natural science of human behavior more reinforcing than the possibility of other sciences. That jus-

tifies my choice, in a way; but does it explain it in any way?

Your doctoral adviser at the University of Chicago was Jack Gewirtz. In your acceptance address for the first award for Distinguished Service to Behavior Analysis in 1997, you said that he "had taught you how to be a doctoral adviser by taking it seriously—very seriously." What did you mean by taking it "very seriously"?

Jack is and always was very, very smart; he could learn any body of knowledge, analyze any theory, pursue any argument, solve any logical puzzle, make sense of nearly opaque prose, and master any techniques—and fast. I admired all that; I thought that was what scientists did. Or should do. And that I could learn it from him.

I admired the calm competence with which he made his transition from the Hullian behaviorism he had been taught at Iowa to the operant behaviorism he was teaching himself, mainly through reading and to some degree through discussions with his Chicago colleague, Howard Hunt. I admired the fact that he deliberately compared both theories' accounts of one problem after another, using mainly the criteria of adequacy, parsimony of explanation and prediction, and verifiability, and only then decided that operant was better.

He was very serious about advising me, as he was about advising any student, pre- or postdoctoral, then, later, and now. Being serious meant a number of things:

I got a lot of his time—not just an hour now and then, but however long the current topic required, with or without an appointment. I was to learn everything he knew I should learn, and quickly, and well; all that was checked. He told me what to read, and how to find more than that—much more than that—on my own. I was not allowed to own a mistake any longer than it took to voice it in his presence; he would correct it immediately, rationally, and insistently. I was never bullied into an agreement; when my behavior had to change, it changed not because Jack

wanted it changed, but because the logic and facts he put in front of me required it changed—in me as they had required it changed in him, earlier. I could always leave Jack's presence, of course, but I could not escape his arguments; they were the kind I would brood over wherever I was until their consequences were clear.

Jack did not see our relationship as friendship—or, for that matter, as enmity. He saw it as teacher—student. We could like each other or dislike each other, but either way, Jack was to teach and I was to learn—and I was to be probed from time to time to see if by then I could teach a little as well. And if I could, I was probed more and more often, to see when I might have anything of value to teach. And if I did, not me but what I taught was admired briefly, after which its logical consequences were pursued at length.

It was clear that for Jack science was neither a game nor a hobby; it was most of life, and it was—serious. (Is that a mysterious word?) And so science advising was equally serious. Jack had an active, sharp sense of humor, but its occasion was almost always what people did, rather than science.

Clearly, I admired him greatly. And, despite its irrelevance to our relationship, I liked him immensely. And still do both. I know scholars who dislike and ridicule their prior doctoral advisers; I am glad to say I like, respect, and admire mine.

Did you start out as a developmental psychologist and then switch to behavior analysis for all problems and ages?

Jack did not try to make me either a more or less "developmental" psychologist; he only taught me always to ask if there might be a simpler, more systematic, more mechanistic, or otherwise more satisfying conceptual and perhaps even experimental analysis of anything that was called "developmental." I credit him for that, too; it may be standard natural science, but he was one of its two best teachers, in my education. On my own, I began to like the romanticism in the notion that

very-early-life learning might be qualitatively more powerful and more enduring in its effects than later life learning. That was a favorite media theme at the time, and its wistful endorsement was as developmental as I got. For a while I thought that this notion might be added to an operant account, given that I did not find it there already; but with more and more facts and research experience, I eventually abandoned it as unsubstantiated because it was probably untrue.

You wrote that your doctoral adviser, Jack Gewirtz, taught you or "let you learn" about behavior analysis. Was there anyone else who guided you to operant psychology?

My best *first* introduction—before Jack—to what would later be called behavior analysis was the text by Keller and Schoenfeld, *Principles of Psychology* (1950). I read it because another graduate student, not at all committed to a natural-science approach, even so knew it was an exemplar of what I was seeking, and told me to read it before I left the field (which I was threatening to do, in despair at that moment of ever finding a provable psychology system). For me, that text preceded and was much more powerful than Skinner's *Science and Human Behavior* (1953), which Jack later made sure I read.

Jack also made sure I knew that in our department, operant psychology was *done* (in contrast to merely argued) in Howard Hunt's physiological-psychology rat lab. Jack made sure I spent time there asking Howard's students what their researches were; and he made sure Howard knew I was loitering in his lab out of curiosity about what it was like to actually shape behavior. Howard was intensely generous in response: He sought me out, talked to me at length, and quite literally gave me to his most advanced student, William Beckwith, for a Skinner-box apprenticeship. I simply did everything Beckwith told me to do, and that brought my behavior into contact with controlling a rat's behavior by science,

not by art. Not just understanding it, but doing it. That was very powerful. In addition, because I was often in Howard's lab late in the afternoons, when I was done I would hopefully walk by his office door, which was always open, slowing my pace as I got there; he was always inside, typing away. He would look up and say, "Sorry, I can't talk now [not that I had asked, only wished]; too much to do for tomorrow!"—and then he would almost always say, "Oh, come in anyway." I would, and he would talk to me about science, experimental analysis, psychology as a profession, publishing, universities, *this* university, teaching—about everything relevant to science, science training, and profession. That too was very powerful. And I was not even his student.

You said you almost left the field or threatened to; whom were you threatening? Did you have any idea as to where you would turn?

I had entered psychology after reading a book on experimental psychology. I had no psychology background, so the department required me to take nine low-level courses before beginning serious graduate study. The first eight of them did not present psychology as a systematic natural science; they relied heavily on theories that seemed neither confirmable nor disconfirmable. I began to think I made a serious mistake; I told myself I might have to leave for some other field, probably biology. Fortunately, that was the moment when I was required to take Jack Gewirtz' course on personality; and fortunately, he chose to teach it, for the first time, based on Skinner's *Science and Human Behavior* (1953). That book and that course showed me, just in time, that there could be a systematic natural science of behavior. Then I read the Keller and Schoenfeld (1950) textbook, which showed me what the actual experimental research in such a field would be—and also showed me two behavioral scientists restricting their verbal behavior almost entirely to what their experimental data

could prove. That strongly reaffirmed my decision to enter psychology and stay there.

You and Sid Bijou wrote three books on child development. How did you and Sid come to write these books?

When he recruited me to the University of Washington, Sid Bijou remarked in a letter that after I came, we would discuss the possibility of making a comprehensive “two-factor” account of child development. In my innocence, I responded eagerly, saying I would like to try fabricating a comprehensive “one-factor” account. I thought he meant we would talk. We did. But after a few years, Sid said our weekly meetings and discussions had come to the point of making them into a book, and that he already had a publisher. I was terrified by the prospect. But Sid has always been a person who not only exuded confidence in his project proposals, but also always succeeded in them. So I agreed, trusting he would teach me how to write a book. He let me find out what worked for me by myself. We assigned ourselves certain sections of the total book; wrote them; exchanged them for editorial suggestions for rewritings or additions; and sometimes did, and sometimes did not, take each other’s advice.

Notice how generous that was: Sid could easily have written all of Bijou and Baer by himself, and faster than our collaboration required. His insistence on my coauthorship was purely a gift to me, at some cost to him.

You said that Sidney Bijou recruited you and Montrose Wolf to the University of Washington. Was this right out of school (University of Chicago)?

Bijou recruited me in 1957, fresh out my doctoral training at the University of Chicago, to be a tenure-cycle assistant professor. Wolf was recruited by Bijou in 1959 or 1960 from Arizona State, to be a nontenure-cycle research associate.

So, Mont Wolf consulted on research projects, some at Rainier State School, and you taught developmental

psychology courses and wrote those books on child development. What else did you do?

I also advised doctoral students. I tried to do it as seriously as Jack Gewirtz had done it. And I served on a series of university committees; none were memorable. Except for a moment when a dean tactfully explained I had been appointed to a certain policy-making committee in error, and perhaps I should resign: I told him my only regret was that I had but one committee to give up for my university.

Wolf and I were both very busy with our official assignments, but we still found time to talk. Some of the talk was of wistful reminiscence about our earlier trainings in the animal laboratory. So, more or less as recreational therapy, we set up a Skinner box for a fish, and showed that fish were capable of very elegant temporal discriminations under fixed-interval schedules of fish-food reinforcement. That confounded a thesis recently advanced by an eminent comparative psychologist, which made us smug, perhaps confirming what a good recreational therapy it was. Besides, it let us present the data to a convention of the American Psychological Association (APA) as a study of temporal discrimination in a fish caught by a Wolf and a Baer.

One of the best things we did was attend Sid Bijou’s every-Friday seminar. There were a small number of us, depending on the year: (in alphabetical order) Baer, Bijou, Birnbrauer, Erickson, Etzel, Hall, Hart, Lovaas, Orlando, Risley, Sherman, Steinman, Wahler, Wetzell, Wolf, and any other interested graduate students. Sid required each of us to describe our current work, research plan, hypothesis, or conundrum, one person per week; everyone else was free to comment critically and constructively—or to be silent. Mostly we all talked; it was a good environment. We learned a great deal every week. Sometimes it was a better way to conceptualize what we were doing, sometimes it was new things we could do; sometimes it was new things

that were better not done. Occasionally, we discovered that two people can come to different conclusions looking at the same visual data display; to me, that was especially valuable training.

Why did you leave the University of Washington for Kansas?

The Washington psychology department fell into a quite standard academic civil war, dividing itself on issues of teaching mission, graduate training, the criteria for faculty merit, and personality. Each faction felt itself perfectly correct and the other faction perfectly incorrect. The 24 members of the department divided reliably into one group of 8 and another of 16; I was one of the 8. A split of 16 to 8 meant that the 8 would always lose, and on issues that mattered a great deal to them (for example, the legitimacy of single-subject design for student dissertations). Seven of those 8 eventually left. I have always considered my 8 years with Bijou at Washington the second-best professional event of my life—and my consequent departure for Kansas the best professional event of my life.

Leaving Washington, I had several options. I chose Kansas because it was an opportunity to help build a new department, one that could recover much of the intense intellectual research and teaching community that Bijou had built at Washington, and that might well go farther.

You've had many students over the years. Who were your best students? Why?

I will disappoint you by refusing your request to rank-order the importance or value of my doctoral students. Just as it would be devastating to my children if their parents valued them differentially, so it would be with graduate students.

In their book Theories of Personality (1970), Hall and Lindzey said that a large number of gifted students, such as Nathan Azrin, Ogden Lindsley, Donald Blough, William Estes, Norman Guttman, Richard Herrnstein, William Morse, and Herbert Terrace,

increased Skinner's impact on psychology and extended his work in the vanguard of psychology. His students kept experimenting with and extending the tenets of operant psychology (p. 478). Do you agree?

A graduate student should be trained to become an *independent* scholar, researcher, and thinker. If students continue to work on the problems of their mentors, it should be by choice, not necessity. That many of Skinner's students extended his work is not necessarily about Skinner. In my opinion, the ones you named were indeed independent scholars; although they extend Skinner's work, I think they did so as a continuation of the natural science of behavior, not as a continuation of Skinner. That, I suspect, is how Skinner saw his own work.

You have received many honors and awards. What do you consider your greatest achievement?

There are two contexts: my profession and my daughters.

Profession: Given that I am healthy, comfortable, and in no danger of death, which has almost always been my case, my best reinforcer is to understand. Understanding has rules; for me, they are the rules of natural science. My best reinforcer is to gain a natural-science understanding of any phenomenon. Since 1952, I have wanted most to gain a natural-science understanding of behavior—of why organisms do what they do. My greatest achievement is that for the past 48 years, I have been in wonderful positions to do that, and I have used those positions to understand more about behavior.

I am, I fear, not the usual good citizen: I did not get into this to make the world better, or to help worthy people resolve terrible life crises. I am not the usual materialist: I did not get into this to be rich or powerful. I am not the usual narcissist: I did not get into this to be famous. I am not the usual communalist: I did not get into this to teach the world the joys of scholarship.

I got into this to understand behavior; to do that, research is the best way,

and teaching is the close-second-best way. Publishing shares the results, of course, but mainly I have published as a survival skill (first for tenure and then for renewals of research funding), and to teach my students a survival skill, and to clarify my understandings—not to share. (I am glad to share, but in my experience, very few people read.) Thus, despite my teaching almost more than 100 doctoral students so far, publishing even more often than that, holding distinguished-professor rank and several awards, and having been research-funded almost all my professional life, the functional analysis is that this has been a personal adventure in maximizing *my* understanding.

So, for me, my greatest achievement was to get much of what I came for. If any of my achievements have value for my society, *it* will have to say what they are.

Daughters: I am extremely proud and grateful that my three daughters have achieved good, happy, self-maintaining, fulfilling, healthy lives. I wish I could take a lot of credit for that, but doubt that I can; their mother deserves the greater share. But for those outcomes, I am happy; and for my part in them, I am proud. This has never looked to me like a society in which children just automatically turn out well. Thoughtful effort was invested. Apparently it was good effort.

You dropped your membership in the American Association for the Advancement of Science (AAAS) in 1968, the Society for Research in Child Development (SRCD) in 1979, the Association for the Advancement of Behavior Therapy (AABT) in 1980, APA in 1986, and the Society for the Experimental Analysis of Behavior (SEAB) in 1996, but you have retained your membership in the Association for Behavior Analysis (ABA). Why? I am particularly interested in why you dropped your memberships in APA (Divisions 7 and 25) and AABT, as I think they reflect the clinical aspects of applied behavior

analysis. Perhaps you do not see them that way. Please elaborate.

Early in my career, I joined all the seemingly relevant associations, on two assumptions: (a) They would prove educational. (b) Those memberships would be good credentials. Both assumptions proved incorrect. No one whom I needed to care about me ever cared whether I belonged to those associations. The only thing I learned from AAAS, AABT, SRCD, and especially APA was that the overwhelming majority of their members did not have the same definition of science that I did. I wanted to discuss science, but rarely could I find useful conversations. I did not want to discuss the politics of practice or of the association, but those were the conversations that were abundant. With time, I became not only bored with APA, but increasingly ashamed of, or uncomprehending of, the variants of psychology it endorsed, and with which I was stigmatized in society because I bore the professional label of psychologist. In addition, all that travel, hotels, restaurants, and taxis proved expensive; I felt I was not only not getting value, I was spending money and time on events I increasingly disliked. The single exception was seeing old friends and former students again. But as I tried to make clear to them, I would much prefer an ongoing correspondence with them to a constantly interrupted once-a-year visit in some noisy bar or restaurant. (Perhaps 10 of them took me up on that.) At first, I let research grants pay for those things; but as it became clear that I was gaining almost nothing of value, that seemed immoral to me, and for the past 10 to 15 years, I have used my research funds to pay some of the costs of my students, if they are presenting work at the convention. *They* need the professional visibility, the opportunity to decide if there is anything of enduring value to *them* in continuing their membership. I lecture them a little on networking techniques and potential benefits before they go.

The single exception is ABA. Nothing it does shames me; all of it is understandable; most of it is interesting; some of it is new to me; and almost everyone there who has a conception of science has the same one I do—we are almost all natural scientists. My students almost always choose ABA as well, although I do not dictate that choice.

At first, it took me a few years to believe that ABA was what it was. My first view of it was as a playground, and I did not attend, preferring other arenas for play. Soon the accounts of attendees showed me I must be wrong, and I have been attending gladly ever since, with the (principled) exception of the Orlando meeting.

Incidentally, membership in SEAB is by invitation of the existing members, who rarely exceed 50 in number, and is limited to an 8-year term. The main function of SEAB is to publish and supervise the *Journal of the Experimental Analysis of Behavior* and the *Journal of Applied Behavior Analysis*, which is principally what its members discuss. Thus, I did not drop my membership in SEAB; I finished my terms. I was honored to be asked to serve twice, which is rare, and I did; but I considered both terms an honor and a duty rather than an educational experience.

You were a visiting professor at the University of California at Los Angeles (1964); the University of Hawaii (twice, 1967 and 1971); Brigham Young University (1969); the University of Western Australia (1971-72); the University of Sydney, Australia (1972); San Francisco State University (1975); Universidad de Sao Paulo, Brazil (1975); the University of Arizona (1977); Drake University (1977); Keio University, Tokyo (twice, 1982 and 1985); The Ohio State University (1988, 1994, 2000); Kwansei Gakuin University, Japan (1995); Universidad de Almeria, Spain (1996); and Universidad de Goiania, Brazil (2001). In addition, you were visiting researcher for the Japan Society for the Promo-

tion of Science (1982) and Visiting Researcher for the Japan Society for Mental Retardation (1985). Now, how does this work? Did you apply for these positions? Did they seek you out? Were they honorary appointments? How can you be in three places at once?

I did not seek out these invitations to teach elsewhere, but I welcomed them. They were opportunities to see more of the world with a fair amount of financial subsidization, and with either the blessing or the tolerance of my employer, the University of Kansas. Perhaps by accepting almost every invitation I received, without negotiation, I established some reputation as an easy touch, which perhaps led to more and more such invitations. Each of these hosts invited me to teach the system of behavior analysis, or its applications, or its research methods: I always felt competent to do that, so it was always easy to accept. These might be called honorary appointments, especially in the sense that they were to be brief; but they were also real enough to offer pay, expenses, an ID card, and a title. My way to work for several universities at the same time is to teach my major courses at Kansas for twice as many hours per week as the credits they could carry would require. That way I can be gone several weeks of each semester, yet I will still have taught my students for more hours than they paid for.

I've been told that writing and publishing books rather than journal articles is the best way to increase one's visibility. Few people read scientific articles. Is it more beneficial to write books because people more readily read books; you can pick them up and can see the author's name and photograph on the jacket? Having done both, what do you think?

I wrote books and articles at first because Sid Bijou said I must if I wanted to survive and be funded in this profession, and I believed him. (He was right, of course.) Subsequently, I wrote them mainly to get my students into

print—to do for them what Sid did for me. If I have any other reason these days, it is to educate myself and my readers. I am certain that if I write something new, I will teach myself something in the process, so I write a lot, most of which doesn't see print; and the probabilities I am about to cite are my best guess (and only a guess) at the likelihood of educating anyone else if my words do see print.

Books are read mainly by undergraduates, of whom 1 in 50 can be jarred out of an intellectual rut by what they read; and of that 2%, perhaps 1 in 10 will be, by any given book. Some journal articles get read by some graduate students and professors: Of the graduate students, perhaps 1 in 10 can be jarred into something new by the article; and of the professors, perhaps 1 in 20 can be.

Those were the probabilities for me as a student, and I think they are the probabilities for me as a professional. When I was a graduate student, there were a few articles that amazed me and drastically changed my course; those articles not only convinced me that I should behave differently now, they also taught me that it would be worthwhile to write such articles in the future, if I could, because there were always a few people who could read them and be changed.

Visibility? The crucial visibilities for me were with my employers and my grant-application reviewers. As best I could tell, journal articles were the best way to become visible to them, books second best, and convention papers a far-distant third. Not that they necessarily read my output; my employers only counted them and asked about the prestige of the journals or the publishers, and my grant application reviewers counted them and sometimes read and evaluated them. But my most important point is that I no longer care about visibility; I still care about understanding more, as I always have, and writing is essential for that. Published writing is not.

Perhaps a functional analysis is that

understanding is my best reinforcer. When professional survival was essential to that, it was thereby just as strong, as long as it was true. But visibility = fame was and is a very weak reinforcer. Indeed, if you are interested in my view of it, it is an extremely thin schedule of very brief external-environment events (the occasional invitation to do something exceptional; the occasional eloquent introduction as a "world authority" or some such; the very occasional award). If you want to be famous, you must collect those very rare external-environment events, treasure them, and constantly self-instruct that surely they mean you are steady-state famous. I've tried that, some years ago; I found it hard work and boring.

Over the past 40 years or so, you've seen the field of behavior analysis grow and evolve. Could you comment on this? Where have we been and where are we going?

I don't know. I don't have a sense that we propel the discipline, only that we can watch it go where it goes and go with it.

I note that behavior analysis and applied behavior analysis have been very small, heavily criticized, and despised disciplines; I expect that will continue, largely because proof-dependent natural-science views of human behavior are both threatening to their competitors and also very poor show business. I expect that the powerful techniques of applied behavior analysis will always be quickly stolen from their context and their terminology, made mentalistic, psychodynamic, or quasineurological, and used widely by practitioners who credit their effectiveness to cognition, inner motivation, or neurology. I predict that effectiveness will always be less in their hands than it would be in the hands of proof-dependent functional analysts, but because the techniques were developed in functionally analytic, proof-dependent hands, they will maintain some effectiveness even in clumsy, nonanalytic hands.

Given this pessimistic view of our place in our society if we continue to be a proof-dependent natural science readily exploitable by good-show-business rivals, our major problem will always be to survive as a small and despised discipline. (It may be that I can see it easily this way because I am a Jew. I know that kind of survival is not much fun, perhaps—but always possible.) As best I can see, that kind of survival requires universities that continue to act like universities. Universities are—at least, were—places where you can convince at least a few students to always value proof above show business; then you can display behavior analysis as well as the other natural sciences to those students, and at least some of them will choose it, and thereby maintain it and extend it. It seems that the leaders of universities are urging them these days to become educational corporations. It is the nature of universities to change slowly, if at all, so perhaps there is little to worry about. But if universities do become businesslike, they may become inhospitable places for an insistence on proof, at least in the behavioral sciences. It doesn't sell.

Certification of behavior analysts is becoming widespread in this country. Certain states, such as Florida, California, Texas, and Oklahoma, have adopted certification. There is a national Behavior Analyst Certification Board, Inc. What do you think about this?

I have a principled opinion and a pragmatic opinion about the certification of applied behavior analysts. My principled opinion is that certification in any discipline rigidifies its practice and knowledge acquisitions, which is bad. It is sometimes difficult to get applied behavior analysis into useful practice, because its practitioners are not always certified clinical psychologists, speech therapists, physical therapists, school teachers, or whatever is required, which means some deserving people do not receive what might prove to be the best treatment as soon

as they might, if ever. That is bad. Certification slowed the validation of applied behavior analysis; that was bad. It will slow the future extension, refinement, and especially any revolution of applied behavior analysis, even when data based; that will be bad.

My pragmatic opinion is that the certification of applied behavior analysts will let them be paid more often than otherwise, and give them a useful professional and legal status in our society, which is good.

I was delighted to read your article with Mont Wolf and Todd Risley, "Some current dimensions . . ." (1968). I was even more excited to read the sequel 20 years later, "Some still current dimensions . . ." (1987). Please comment on these hallmark articles.

You may be my favorite reader. Many readers showed in many ways how much they liked the 1968 statement; very few showed in any way that they liked the 1987 version—indeed, almost the opposite. There was nothing wrong with the 1968 paper, other than naiveté; the 1987 paper was as identical as it could be, given that it was 20-years-worth more sophisticated. The seven dimensions seemed simple in 1968; they seemed complicated and contextual in 1987. They seemed that way because, in truth, they are complicated and contextual, and we had finally learned some of that complexity and contextual qualification. I was delighted to write the 1987 paper; it let me make real, explicit, and clear almost everything I had learned during those 20 years. But I have had a strong sense ever since then that the readers of the 1987 paper disliked its complexity and contextualism—that they wanted the simplicity of 1968 reaffirmed once again. Too bad I couldn't do that for them.

In addition to you, I consider (in alphabetical order) Ted Ayllon, Nate Azrin, Albert Bandura, Sid Bijou, Vance Hall, Leonard Krasner, Og Lindsley, Ivar Lovaas, Jack Michael, Gerald Patterson, Todd Risley, Beth Sulz-

er-Azaroff, Leonard Ullmann, and Mont Wolf to be pioneers in behavior analysis. This list does not include people who are deceased. Please comment on this list. It is mainly the same list as the one used by Goodall in Psychology Today (1972) titled, "Shapers at Work." Do you remember it?

Your list reflects an even more sexist field than it was at the time; the women were there, but mainly were ignored. In particular you should not miss Barbara Etzel and Eve Segal, in my opinion. Too bad you cannot interview the recently deceased Ellen Reese, who would have been both a superb and a *de rigueur* choice—but you could interview Jane Howard, her former student, about her; and after you ask Barbara Etzel about herself, you could ask her about Ellie, too. And ask her whom I have missed.

In my opinion, you are missing many people who made what I call pioneering contributions to technique or conceptualization. Some did so more than once or twice; others did so more frequently. But the concept of pioneer does not seem to require repetition, does it? To blaze one trail is to pioneer; to blaze many trails is to pioneer repeatedly. Are you restricting your study to repetitive pioneers?

I appreciate that your problem is to distinguish between people called "pioneers" and people who were only passing through the area at the time, or stayed in it but were unable to or were careful never to step out from behind the pioneers and explore a different part of the newly opened territory.

Please remember that the *Psychology Today* article was a journalism event, not a science event. It was well done, but the criteria that guided its choices were journalistic and above all hasty; they were neither scientific nor just, nor even thoughtful, other than by coincidence.

You have such a passion for science, particularly natural science. Why? Where and when in your training did you start to feel this passion?

I don't know why understanding is

one of my best reinforcers. It may have begun in an unusual relation between my mother and my school teachers. My mother readily and fully explained every curiosity I ever brought to her; she was pleased I was curious and certain she knew the answer. Later, my school teachers explained many of the same curiosities very differently, but with the same certitude. So, my schooling indirectly made me look for a "correct" way to choose between conflicting certitudes. I call that way understanding.

Public school did not offer the way, but it did require science courses. Those courses said science had proofs its certitudes were correct; that meant something called proof might be the way to understanding. Unfortunately, these science courses did not teach me the rules of proof, but once in a while they did program a "demonstration." I concluded that if you could see (hear, feel, smell, etc.) a relation again and again, it was proven to be true. I apparently supposed anything I couldn't observe repeatedly could not be proven to be either true or false.

The University of Chicago eventually showed me the illogic of that conclusion, and it required me to learn a variety of other ways to define and determine knowledge. I understood them, but preferred the natural-science way. I had become certain that the only justification for certainty was a natural-science proof—and I was wryly aware that I had no natural-science proof of my certainty that knowledge required a natural-science proof.

I can describe that behavior change, but cannot identify the variables that controlled it. One of my colleagues remarked that despite four decades of repeated exposure to changing fashions in behavioral science research, I haven't changed. (It may or may not have been a compliment.) Apparently, those exposures did not contain the controlling variables.

Throughout this interview you have spoken about being Jewish. It is evident that being a Jew was (is) very im-

portant to you. I've known behavior analysts who were devoutly religious. Do you think being a behavior analyst (scientist) is antithetical with being religious? Are you a practicing Jew now?

I was raised by two Jewish atheists. They taught me nothing of the religion or its practices. They said we were Jews, to enter that word in the religion blank of all application and information forms, and to expect trouble. When I was a school child, my Christian peers told me often that my being a Jew made me aversively different from them. They could not define the difference, and did not like being asked. That was essentially the importance of Jewishness to me.

As an undergraduate at the University of Chicago, I was required to read the gospel according to Matthew. It was a revelation (you should excuse the term). So that's what Christians believe! Two things became clear: Christianity was an admirable ethical code; and if Matthew understood Jesus correctly, and if the teachings of Jesus defined Christianity, then none of my public-school peers had been Christians. At least, they had not loved their neighbors as themselves, they did not return good for evil, and they did not treat the least of them as they would treat their God. They had claimed they were Christians because the crucifixion of Jesus had got them off some hook, but they could not define the hook and again did not like being asked.

It seems to be true that the importance of my being Jewish is the importance my peers and my society place on it. When no one else is interested, neither am I. An exemplification of that adage became very clear when I was in Brazil some years ago, and was introduced to a gathering of long-ago expatriate European Jews. For an hour, they gently, courteously, and delicately probed my degree of observance. At the end, one of them asked how much of a Jew I was, and the group fell silent. Without a moment's thought, I said, "Not much of one—

just enough to be killed for it." The point is not that I said that, or said it without thinking; the key point is that as I said it, the entire group nodded approvingly, and after that we discussed our various professions.

I see science as a collection of rules for finding out what can be proven. Any proof is as good as how closely the rules were followed. And the rules often are debated as such, because they are *our* rules: To some degree, we define ourselves by those rules. I see religion as a collection of beliefs about the universe; a collection of rules for how to behave otherwise; and a grand absence of debate about the beliefs and the rules because they are God's facts and God's rules: To redefine the facts or the rules is to redefine God. (Which I notice is a frequent behavior among people inconvenienced by the present rules.)

You cite some scientists as people who follow both sets of rules. That does not seem surprising to me: Conditional discriminations are very common in everyday life. For example, if I am going to buy a car, I discriminate manufacturer, year, model, price, cost of insurance, economy of operation, economy of repair, esthetics, color, utility, reputation for durability, and so on. But if I must cross the street to get from one car dealer to another, during that crossing all those discriminations are useless; I will discriminate only the cars' directions of travel, their speed, their distance from me, the width of the street, and how good my footing is. Once across the street and in the new dealer's shop, suddenly all the former car discriminations are relevant and investigated again, and the latter are forgotten as irrelevant. Isn't that a reasonable metaphor for people who do science some of their time and religion some of their time?

You say some people abandon their religions because of their commitment to science. Apparently they want their science rules to cover everything. I don't see the natural-science rules as meant for that. For one thing, I don't

see them as true; I see them as a prescription to try, which I am following to see what happens. I like what happens; the resultant knowledge has a nice, orderly, useful structure. But nothing that has happened so far tells me that if I had a religion, I should abandon it.

I teach Research Methods every semester. I sum up certain scientists' devotion to proof in a motto: "I will believe anything we can prove, and nothing we cannot or have not." I also remark that I know of no one who can actually live by that motto all day, every day. Even so, I make clear how much I admire it.

I note that some people try to define a set of meta-rules that will subsume without contradiction their rules of science and the rules of their religion. That seems an interesting intellectual challenge, and I wish them luck. I certainly will admire any construction that accomplishes that goal, if it's not too complicated for quick study. But that amalgamation is not one of my goals, so cost-benefit logic will sharply determine how much interest I give any example of it.

I understand your reasoning behind putting your students and children as your greatest accomplishments, but I'm looking for something else. Something that would indicate a "scientific discovery," or something that led to a "breakthrough" in behavior analysis, something that people would point to and say, "Don Baer did that."

None of my training that I admired was about being the first; it was about being correct. I've done some studies that had been done earlier by others, but unconvincingly. I wanted to know if they were correct, so I redid them in what seemed to me a better way. I sometimes thought I was doing something original and unprecedented, and that my analysis must therefore be important, but it always turned out I was just ignorant of some part of the literature. I've never had an idea someone else hadn't had earlier. To go only a little too far, I doubt that any of us has.

If there is a personal accomplishment I would like to claim, it is in other people's behavior. I would be gratified if they could ever say that something I wrote or said let them finally understand a problem that had been puzzling them. I have always valued those scientists who taught me in that way, and I have wanted to be one of them, now and then.

It is often said that one's children carry on a person's legacy. In the case of professors, students keep their work alive. Do you think this is true?

Yes, my doctoral students are my legacy, if I have one. (*Legacy* may be more difficult to define than *serious*.) If we know what a legacy is, they are it much more so, in my opinion, than anything I have written or might ever write. They could have bigger and more enduring consequences. Indeed, almost everything I write results from teaching my graduate students, and almost always is coauthored with them.

Conclusion

Don received many awards and honors. Among them are the Roy A. Roberts Distinguished Professor of Human Development and Family Life from the University of Kansas (1975), the Don Hake Memorial Award from Division 25 (Experimental Analysis of Behavior) of APA for "exemplary contributions to basic behavioral research and its applications" (1987), the Burlington Northern Foundation Award for "faculty achievement" from the University of Kansas (1989), an award from the Edna A. Hill Child Development Center at the University of Kansas for "intellectual leadership and guidance in research endeavors" (1993), the Award for Outstanding Contributions to Behavior Analysis from the Southern California Association for Behavior Analysis (1995), the Edgar A. Doll Award for "outstanding contributions in behavioral science on behalf of people with developmental disabilities" from Division 33 (Mental Retardation) of APA (1996), the Life-

time Achievement Award for "pioneering and outstanding contributions to the conceptual analysis and application of behavior analysis" from the Florida Association for Behavior Analysis (1996), the first Distinguished Service to Behavior Analysis Award from the Association for Behavior Analysis—International (1997), and the research award of the American Association on Mental Retardation "for significant lifelong contributions to the body of scientific knowledge in behavioral science and its application to the field of mental retardation and developmental disabilities" (2001).

In addition, Don was a fellow in Divisions 7 (Developmental Psychology) and 25 (Experimental Analysis of Behavior) of APA and in the Society for Research in Child Development. He was also a past president of the Association for Behavior Analysis—International (1984–1985) and a three-time Distinguished Guest Faculty Member in the Applied Behavior Analysis Program (College of Education) at the Ohio State University. Don published eight books, 62 book chapters, and 138 journal articles, many of which were reprinted in books and other journals. Many of his writings have been translated into other languages. Don was not only a pioneer in applied behavior analysis but also one of its greatest teachers and researchers and one of its most prolific writers and speakers.

Don Baer died on Sunday, April 28, 2002. On October 25, 2002, he would have been 71 years old. We'll miss him.

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